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## PRIORITY AND PRACTICAL ENTOMOLOGY.

By

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In the Address of the President of the Entomological Society of London, at the Annual Meeting for 1907, the following sentence occurs: "For the last fifty years, names have been constantly changed, and there does not seem to be any immediate prospect of settlement." The paragraph in which this occurs (Proc. Ent. Soc., 1907, pt. v., p. cv.) deals with the question of priority in the names of insects. To a purist in nomenclature, who devotes time to finding the original descriptions of the oldest systematists' species, the correctness of the name to be applied to an insect is a matter of vital importance; this importance is purely academic and bears the same relation to practical entomology, that the variation in nomenclature of geologic strata does to the working prospector or mining surveyor.

There is a very large number of practical working entomologists who are engaged in dealing with insects as pests or as sources of benefit to mankind; these work over the whole globe, most in places far from the academic centres of the world. All correlation in work is based upon accurate diagnoses of insects, and it is all-important to them not only to correctly place and class their insects *on an uniform system applicable to all*, but to *use the same names throughout*, so that one working entomologist in Borneo say, reading the last bulletin on the "American Bollworm" from the United States, may know absolutely whether that insect is identical with any one of those he is familiar with, as it probably is.

I take this as a specific instance; we all knew the American bollworm as *Heliothis armigera;* there is a great literature on it, some extremely valuable. Quite lately, the British Museum issued part of a "Catalogue of Lepidoptera Phalaenae," a monumental work which deserves the gratitude of all entomologists. In this work, issued from

our National Museum, we find our old friend called *Chloridea armigera* in the text, but a few lines in the Appendix call it "*Chloridea obsoleta*." I would ask who is the better for this change; could not the author have stated that the specific name should, on strict priority, be *obsoleta;* could he not arbitrarily have used the generic name *Heliothis* to cover this group simply on the ground of its constant use during the last century, while stating that *Chloridea* was correct; it is no more an arbitrary thing to do than to state that he can identify this species with the species *obsoleta* of an older publication with which probably few systematists even will agree. Then think of the boundless confusion caused; we have all the old literature of economic importance under "*Heliothis armigera*"; we have now *Heliothis obsoleta*, *Heliothis armigera*, *Chloridea obsoleta*, *Chloridea armigera*. We are making nomenclature an end and not a means, and the level of the Science is sinking below that of stamp collecting. It may be said, why use the British Museum's Catalogue; to economic entomologists there is not time to go into these questions of priority except in so far as they affect literature; an authoritative catalogue, such as that of the British Museum should be, would naturally be the standard of an economic entomologist, who must work from the catalogues of systematists. If these catalogues are not authoritative, what is the use of them?

I have quoted at present one glaring instance. Their number is legion; in the Coccids, for instance, *Lecanium*, *Mytilaspis*, *Dactylopius*, *Coccus*, represented very clear groups recognisable at sight; but they have become *Coccus*, *Lepidosaphes*, *Pseudococcus*, and *Dactylopius* respectively (to a large number, but not to all authors) on the ground solely of Mrs. Fernald's discoveries in priority; the poor student of *Coccidae* must learn these before he can benefit from both Nineteenth and Twentieth century authors, and in a recent Memoir of this Department, Mr. Green has to state that he maintains the old nomenclature and give the equivalents. This is a case as flagrant as any, since the *Coccidae* are notoriously important and since these generic names meant something definite to the student of economic entomology.

One may reasonably advance the view that the published work on the biology and economics of insects was just as important as the first or the second description, and the fact that a name has in economic literature represented a definite insect for half a century should out-weigh all considerations of academic priority. Yet it is perfectly clear that this view is not acted upon by even so practical a Department as that of the United States, which, in Bulletin 53, listing the economic-

ally important insects exhibited, have, in over thirty instances, to say "formerly known as ——— — – – —"; this is a catalogue designed for the public, not for the technical reader. If one wishes to identify *Heliothis armigera*, *i.e.*, to be able to apply to it a name by which it can be recognised by others in other countries, one does not delve in the old literature; one looks up whatever manual there is and uses that, with a reference, if necessary, to that manual as containing a description whereby the two workers may check their insects. But with the present arbitrary changes, due to priority, made on the authority of a single author, one must have the latest literature, one must give a list of synonyms, and in all our reference works (*e.g.*, Zoological Record) it will not be sufficient to refer to an insect by one name, but by several, as thus:—*Chloridea* (*Heliothis*) *obsoleta*, F. (*armigera*, F.). I would bring up also other notable instances; we all know the shorthorned grasshoppers and locusts as *Acridiidae*, the longhorned grasshoppers as *Locustidae*; it is unfortunate, perhaps, that the two were not originally transposed, but it emphasises very well to a class of students what nomenclature means when one explains why Locusts are not in the family *Locustidae*, and in this way the transposition of names is useful. But we are now told, by another British Museum Catalogue, that what we knew as *Acridiidae* are *Locustidae*, that the old *Locustidae* are *Phasgonuridae*, and that the *Gryllidae* are *Achetidae*, in spite of the fact that the notable Orthopterists, Brunner, Saussure, and Bolivar, found no occasion to change the old names. Suppose these to be adopted; the student has some text-books with one set, some with another, and has to learn the relative uses of them before he can get to his real work. If any branch of the science benefited, it would matter less, but none does.

I write as a "practical entomologist" (to quote Mr. C. O. Waterhouse) who has to deal with injurious insects, who directs men studying the live insect in the field and insectary, and who teaches students; it is a constant burden finding out the equivalents in different countries of the important insects, it has to be taught to students if they are to use literature at all, and it adds a needless complexity to a subject already sufficiently complex. Furthermore, as our insects are revised by authors at home we have to substitute new names, and these have to be circulated to all our scattered staff so that confusion may be avoided.

In this matter, teachers and practical entomologists alone are concerned; to the systematic entomologist, the mazes of synonymy and priority are (apparently) the breath of life, and the pastime might be a quite harmless one; if one systematist wants to abuse another in

the pages of an entomological journal, no one minds; it even adds an element of farce to an otherwise too sober publication when one Hemipterist has remarks on another. But to practical men who wish to check the growing spread of insects from country to country, who wish to co-operate to deal with big problems, who see in agricultural education the chief solution of these big problems, the question is one of vital importance.

The remedy for this state of things seems to me to lie in the formation either of an association of economic and teaching entomologists, or in the joint action of the various Associations and Societies to form an international committee. Such a committee could direct affairs by correspondence, the different associations doing the work for their own countries; thus for the British Empire, the Association of Economic Biologists and the Entomological Society of London combined could (1) obtain from every economic entomologist a list of the insects he regarded as having a sufficient importance in literature to be "Standardised"; (such a list is in existence in India for practical work, and I presume most entomologists in the Colonies have such working lists). (2) Work these lists into one (where necessary such lists could be easily correlated if specimens were sent, an easy matter if the species is economic and therefore not rare) and prepare a single list giving (*a*) proposed designation; (*b*) designation in use in economic literature; (*c*) designations in use in standard catalogues which contain good descriptions. (3) This list is then sent to the International Committee, who, by taking the advice of known experts in different groups, prepare one list, which is published for comment. (4) The comments are then scrutinised, and where a clear majority of say two-thirds are in favour of a name, it is adopted and used by all who subscribe to the aims of the committee. A reference then to the "International Catalogue" would enable systematists and others to refer to economic literature, and the single reference to the "International Catalogue" would enable every entomologist to know what he was dealing with.

I give below instances of the method of dealing with individual cases; I would point out that a vast majority of the destructive insects are those first found and described; it is just with these that "priority" makes such changes, and the nomenclature of not more than one insect in a hundred of our present recorded species would be touched by the committee at all. The realisation of this will perhaps tend to make the proposal look less revolutionary to pure systematists. I would include in the Committee's scope, the question of family names; practical working entomologists would adopt, not a standard classification, but a standard designation for well-marked families; thus *Bruchidae* are being called

*Lariidae* and *Mylabridae*, solely on grounds of priority; the Committee would, in my opinion, adopt *Bruchidae*.

It is obvious that to regulate such a matter as this must depend upon the mutual agreement of economic entomologists, which will be obtained only by a reasonable policy of compromise. One cannot lay down hard and fast rules; if a rule were made, for instance, that a name in use for the last half century should stand, the literature of 1855-1860 would be sought for to see if or if not the species described came within that limit, and we should have shifted the trouble from the time of Linnaeus to the decade of half a century ago. Mutual agreement would be the backbone of the system, or the chaos of the future arbitrary change or retention of nomenclature would be equalled only by the present chaos, and would duplicate it. There is no alternative that one can see except an arbitrary use of names according to one's own judgment. We have in India a well known pest *Hieroglyphus furcifer*, Serv. Mr. Kirby now finds this to be *H. banian*, Fabr., and he revives this name. Why should we adopt it? All our literature is under *furcifer*, and by retaining that name, no confusion is caused; but, the name may be adopted in Ceylon, Burmah, and other places; readers of our publications will be careful to point out the mistake, and to anyone not up in the question, there will appear to be two species.

Another instance has quite recently occurred; a moth was reared from stored potatoes grown in India in a locality to which seed potatoes had been imported from Italy. The suspicion arose that this was the notorious "Potato Moth" (*Lita solanella*). Mr. Meyrick was good enough to identify it as *Phthorimaea operculella*. What connection is there between these two? If one looks up the *systematic* literature one finds they are the same; but what practical working entomologist can afford to do this in every case and for every species? It involves a great deal of time, a constant purchase of otherwise useless literature, and is a great tax, wholly unprofitable. If I arbitrarily use *Lita solanella*, because my assistants and students can then look it up in agricultural literature, and everyone else uses *Phthorimaea operculella*, how are economic entomologists to know that India is a distributing centre for this pest in seed potatoes, and how are they to take precautions? I think all economic entomologists will agree that we are immensely adding to the difficulties of our work, if it is to be anything more than parochial, either by modifying our nomenclature in accordance with the priority discoveries of systematists or by arbitrarily using the nomenclature we think most suitable. It is impossible for an isolated worker in a far country to do more than offer suggestions; I feel assured it will be for the permanent ultimate good

of our science if we can overcome this growing monster, and I think the Association of Economic Biologists might fitly take up the subject.

It is perhaps premature to suggest that this might usefully be the first problem for the "International Institute of Agriculture" at Rome, as far as it covers insects important to agriculture, since presumably the listing of the pests of all countries will be one of their aims; but, with the support of economic entomologists of all countries, it should not be difficult to fix on an uniform system of family nomenclature and, for each well-defined pest, a fixed specific and generic name.

## EXAMPLES.

(*a*) *Cimex* has been *Acanthia*; is now *Clinocoris* in America, *Klinophilos* in some literature. I would retain *Cimex*, these changes being purely due to priority. This is a splendid example: Bull. No. 47, cataloguing the Exhibit of United States Department of Entomology at Louisiana, dated 1904, gives "*Klinophilos lectularia*, Linn. (formerly *Acanthia* and *Cimex*)." Bulletin 53, dated a year later, listing the Exhibit at Portland, gives "*Clinocoris lectularia*, Linn. (formerly *Acanthia*, *Cimex*, and *Klinophilos*)." Here is a generic change in a popular exhibit and bulletin in one year.

(*b*) *Gelechia cerealella*, Ol., is now *Sitotroga*; this is due to revision of the unavoidably large genus *Gelechia*, and has the sanction of those who study *Microlepidoptera*, as being necessary. I would adopt the name as soon as the Association were satisfied that it was established in general use.

(*c*) *Pulex serraticeps*, Gerv., is now *Ctenocephalus canis*, Curt. If the genus *Pulex* must be split, owing to its including several genera, and if the name *Pulex* cannot be retained for all the "economic" ones (*i.e.*, species on which a literature exists), I would, after an interval, adopt the generic name; the specific change being simply a question of priority, would not be adopted.

(*d*) *Lecanium hemisphaericum* is now *Saissetia*, owing to division of the genus into several. In my opinion, the splitting of the genus is unnecessary, based on inadequate grounds, and I would vote for the retention of *Lecanium*; those to whom the sub-genera really conveyed anything useful could write *Lecanium* (*Saissetia*) *hemisphaericum*.

(*e*) Family Nomenclature.—A change in the designation of a family, on the grounds of a change in a generic name, should not be adopted; the actual names to be applied to Families to be settled and the equivalents listed; *e.g.*, the term *Trogositidae* should stand for the family containing the species known as *Trogosita mauretanica*, and any change would be rejected.

(*f*) *Leucania unipuncta*, Haw., designated, until recently, a pest practically world wide; Hampson, revising the genus, places it in *Cirphis;* the United States Department of Agriculture have adopted *Heliophila*. We have therefore the literature prior to 1900 say, under *Leucania*, the American literature now under *Heliophila*, and the literature of those who follow the British Museum Catalogue under *Cirphis*. This is to me a perfectly clear case where *Leucania* should have been retained for the part of the genus called *unipuncta*, regardless of priority, since it has been used for this important species for so long, and the economic literature under this designation vastly outweighs the importance of the systematic literature.

(*g*) *Psocus divinatorius*, Mull., was the original designation of the common household Psocid known the world over. The original genus was split, and the species passed as *Atropos divinatoria*. The American literature now designates it as *Troctes divinatoria*. In this case, I personally would vote for *Atropos*.

(*h*) *Protoparce convulvulli*, Linn., is the designation of the Eastern "Sweet Potato Hawk Moth," and is now regarded as the correct name for *P. singulata*, the Sweet Potato Hawk Moth of the Southern States and West Indies. Obviously both cannot stand; there is a literature under *cingulata;* there is none under *convulvuli*, and I would vote for the former. The genus has been revised by Hampson, who puts *convovuli* into *Herse*, by Rothschild and Jordan, who fix on "*Phlegethonthius*." If this species is the most important economic species, I would have retained *Protoparce*. If it is not, I would not have retained *Protoparce* for that part of the genus (now split up) which contained the most important species, *if the Committee were convinced the genus must be split at all.*

(*i*) The termite of India is written of as *Termes taprobanes*, Wlk., whilst it is *T. obesus*, Ramb. This is a case of mistaken identification, and the nomenclature must, of course, be corrected, since the two species are quite distinct.

(*j*) *Sylepta multilinealis*, Guen., was the designation of the very abundant Cotton Leaf Roller of India, Burmah, Straits, Ceylon, and Africa. Sir George Hampson, delving in old books, finds it agrees with *derogata*, Fabr. In a case of this kind, since the reference to Fabricius as distinct from Guenee is absolutely no value to anyone, and since the various entomologists of the large area the pest covers all know it as "*multilinealis*," I would unhesitatingly reject *derogata* as useless and frivolous.

(*k*) *Pyrilla lycoides*, Wlk. An amusing interchange of compliments has recently taken place over a species known in India as the

"Cane fly." It was erroneously identified as *Dictyophara pallida*, Don., and this designation adopted in "Indian Museum Notes," in "Indian Insect Pests," and in the "More Important Injurious Insects of India" (Mem. Agri. Dept. India, I., No. 2), in which I tried to fix our nomenclature for the time. In all these there was a clear mistake in identification, and the name was incorrectly applied. Mr. Distant then refers to it as *Zamila aberrans*, and we have now a choice of *Zamila* and *Pyrilla* as generic, and of *aberrans* and *lycoides* as specific names, according as we follow Mr. Distant or Mr. Kirkaldy. To discover all this, we have to refer to three places, "the Fauna of India," the "Entomologist," the "Annals of the Belgian Entomological Society," and there is still a choice of name.

# SOME NEW AND UNDESCRIBED INSECT PESTS AFFECTING COCOA IN WEST AFRICA.

By

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WITH PLATES VIII AND IX.

Family **Capsidae.**

Gen. ? nov. **longicornis,** n. sp.

Pl. VIII, figs. 1 and 2.

*Adult.*—Head buff, mottled with brown. Ocelli two on papilla between antennae. Antennae a reddish brown, the clubs being dark brown. 1st segment short and broad, 2nd segment long and club-shaped, 3rd segment short and club-shaped, 4th segment pear-shaped and short. Beak dark brown, becoming darker towards the apex, four jointed. Thorax, dorsum of pro- and mesothorax, and scutellum of a buff ground colour, covered with dark brown pits and elevations, giving them a rasp-like surface. Wings—elytron, clavus buff, with dark markings and covered with black hairs. Corium buff, covered with black hairs. Cuneus buff, somewhat paler. Cell of membrane and membrane, buff mottled with dark and lighter slate coloured spots. Hindwing hyaline, with clear yellow veins. Legs reddish-brown with darker brown bars. Tarsus two-jointed, distal joint pale. Abdomen broad and flat, light brown, smooth. Length, 11 millim.

*Hab.*—Brafu, Yedra, S. Ashanti.

The female is armed with a long, curved ovipositor, carried in a groove on the venter.

The nymph has a very similar colouration, but the dorsum of the abdomen is of a reddish ground colour, with dark brown rectangular spots raised above the surface.

Very large numbers of these insects were found on the diseased trees and not on the healthy ones. They appear to damage the trees by perforating the bark and so producing "gumming."

Large numbers of nymphs in all stages were found, but no larvae or eggs.

**Cryphalus horridus,** n. sp.

Pl. VIII, fig. 3.

Head a dirty yellow. Beak not apparent, short. Eyes dark purple. Antennae capitate and geniculate, dirty yellow. Elytra and

dorsum of thorax a dirty olive green, covered with transparent stubby bristles arranged in longitudinal lines. Ventral surface a dirty yellow. Legs a pale yellow colour. Third joint of tarsi not lobed, fourth joint visible. The body is cylindrical and thick set. Length, 1 to 1.25 millim.

These minute beetles burrow between the bark and wood of the branches and twigs of the cocoa trees. They must be looked for carefully beneath the bark, as they are not to be found on the external surface of the branch. They make long cylindrical galleries, or burrows, in the deeper part of the bark, and so arrest the flow of the sap. Deep scarring is produced, the leaves turn yellow and fall, and the branches break off very readily at the points of the deepest scarring.

These beetles have caused a great amount of injury to cocoa this year in many plantations in S. Ashanti.

Cocoa is not indigenous to Ashanti, where its cultivation has been recently introduced. Having found that a minute weevil beetle was the cause of the damage to the crop, it became very important to discover the indigenous plant from which the weevils had transferred themselves to the Cocoa. After considerable search, I found similar weevils breeding in the tough outer portion of the Papaw fruit.

It apparently follows from this observation:—

i. That the planting of Cocoa in close proximity to Papaw is undesirable.

ii. That when a Cocoa plantation is once infected with these weevils, Papaws planted among the Cocoa trees might act as a trap for the beetles. This, however, requires experimental proof. I should like also to draw attention to the superficial resemblance between the fruit of the Cocoa and the fruit of the Papaw. Both are very similar in shape and colouration, and both grow from the main stem of the plant.

It would thus seem probable that these beetles are guided, in their choice of a plant, by sight rather than by smell or taste, and this observation may possibly afford some indication of the direction in which, in future, similar search is likely to prove successful.

### **Ceratitis anonae,** n. sp.

Pl. IX, figs. 4-6.

The larvae are reared in the fruit of the Sour Sop (*Anona muricata*) and in that of the Guava (*Psidium cattleyanum*) in S. Ashanti.

*Female*.—Head, front broad, 1/3 width of head, lemon yellow with black orbital bristles. Cheeks fawn coloured. Eyes pale iridescent green, looking yellow, or brown, or blue, in parts, according to angle of

illumination. Ocelli on dark spot, with two ocellar bristles directed forward. Antennae yellow, a shade darker than the front. 1st segment short fringed with black hairs, 2nd segment short, studded with black hairs, 3rd segment three times length of 2nd segment. Arista pubescent, dark brown, long. Palps yellow, 2nd joint club-shaped and studded with black hairs. Proboscis a rusty-red, with fleshy labella bearing brown hairs. Thorax a greenish grey covered with pale yellow pubescence, with three dark brown longitudinal lines indistinct before, but broader and distinct behind the transverse suture, and ending slightly in front of scutellum in broad dark spots. Between these spots and scutellum is a narrow, shining glabrous band of cream or yellow. Pectus a dark brown. Pleurae lemon-yellow; rarely cream coloured. Scutellum, the anterior third, is pale yellow or cream. The posterior two-thirds is shining black, divided into three parts by four golden narrow bands, which unite on the under side of the scutellum, four black scutellar bristles. Legs golden, middle tibiae with dark brown spurs. Abdomen broad, flat, of triangular shape, with well-marked flattened ovipositor. First segment pale buff or fawn, with a narrow dark brown basal band, covered with pale pubescence. Second segment with a dark brown apical band almost the entire width of the segment, covered with black pubescence. Third segment pale puff with pale pubescence. Fourth segment with a brown basal band. Ovipositor golden, first segment with brown apical band. Venter a pale brown. Wings broad and longer than body, transparent, with brown veins, and three transverse brown bands. A brown longitudinal between costa and third longitudinal vein, running from the anterior transverse vein to tip of wing. The middle transverse band has a golden spot, which lies in the first basal and in the discal cell. The longitudinal band has a golden central portion, and the brown edges are accentuated in four places as darker spots. There is in many specimens a faint, short brown band in the second posterior cell. In the basal part of wing, between the first and second transverse bands, there are eight brown dots, roughly arranged in two transverse lines. Halteres cream coloured, squama pale cream. Length, 6 millim.

*Male.*—Colouration similar to female. Front as broad as in the female. The antennae, thorax, pleurae, abdomen, and wings are very similar to those of the female. The legs differ: first pair, femora brown, tibia and tarsus golden; second pair, femora brown with long black hairs on under side, tibia brown with long black hairs on upper and lower edge, diminishing in length towards the tarsus, thus giving the appearance of a feather, tarsus golden; third pair, as in female. Length, 5.5 millim.

The fly is found walking lazily about on the leaves of various bushes, slightly vibrating its outstretched wings.

It is also found on palms where there are coccids.

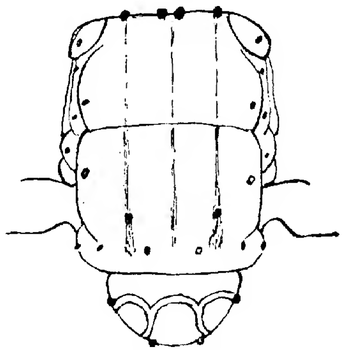

Fig. A.—Chaetotaxy of *Ceratitis anonae*.

Chaetotaxy.—This is, of course, *Trypetid* in character, but I may point out that this peculiar arrangement of the four bristles on the anterior edge of the mesothorax is very easily recognised by unskilled persons, is characteristic, and renders *Trypetidae* readily distinguishable from flies with pictured wings, such as *Ortalidae* and *Sciomyzidae*, to which some of the *Trypetidae* bear a superficial resemblance.

I have added a drawing showing the chaetotaxy.

The *larva* is a white maggot with a black spot and two dark hooks at the head end. It is capable of leaping. When placed on earth the larvae bury themselves and become pupa in two days. The pupal stage lasts thirteen days.

The *pupa* is of a golden brown colour.

The types of male and female are in the British Museum (Natural History).

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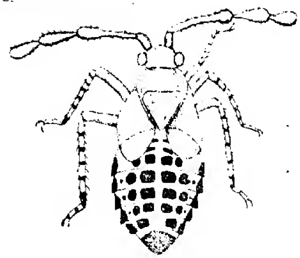

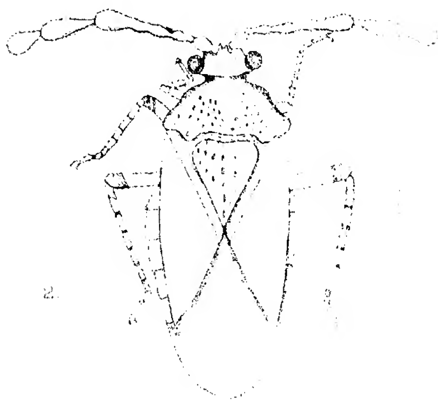

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3.

NEW WEST AFRICAN INSECTS

CERATITIS ANONAE, n. sp.

## EXPLANATION OF PLATES VIII AND IX,

Illustrating Dr. W. M. Graham's paper on "Some New and Undescribed Insect Pests affecting Cocoa in West Africa."

### PLATE VIII.

Fig. 1.—Nymph of ? *longicornis*, n. sp., found on diseased Cocoa plant at Brafu, Yedra, S. Ashanti.

Fig. 2.—Adult female.

Fig. 3.—*Cryphalus horridus*, n. sp. Adult female. × 97.

### PLATE IX.

Fig. 4.—*Ceratitis anonae*, n.sp. Male.

„ 5.— „ „ „ Female.

„ 6.— „ „ „

*a.*—Larva.

*b.*—Pupa.

The small divisions of Scale represent millimetres.

# THE FUTURE DEVELOPMENT OF TECHNICAL MYCOLOGY.[1]

By

EMIL WESTERGAARD, Ph.D.

ONE of the most striking examples of the recent rapid progress of pure and applied science is, without doubt, to be found in the rise of Technical Mycology. From being, for obvious reasons, non-existing prior to Pasteur, this branch of applied science, or rather the numerous branches which we now gather together under that common name, has since then developed into a position of one of the most fruitful and practically important subjects of the present time. The progress has indeed been so rapid, the amount of experimental work and the mass of literature so great, that it would seem not unprofitable to devote a few minutes to a consideration of the present position, and to try, if possible, to form an idea of the probable lines of further development in the immediate future. This would seem to be so much more useful as, owing to the reasons already mentioned, and also, and perhaps to an even larger extent, to the newness of the subject, the term "Technical Mycology" conveys, I am afraid, but a very vague meaning to most people who are not actually engaged in its pursuit.

Whilst the foundation-stone of this new science was laid by Pasteur through his demonstration of the importance of the presence and activity of distinct species and varieties of micro-organisms, it was reserved for Emil Chr. Hansen to show how our knowledge of microscopic plant life could be turned to practical use, and thereby to initiate the science of Technical Mycology as such. His idea, the systematic selection in each individual case of the most suitable type and the exclusion of all others, has, as you are undoubtedly aware, been adopted by the brewing industry throughout the world with the most conspicuous success. The same principle has since been applied to the distillery and yeast-making industry, chiefly by the Berlin school, and to wine-making by Muller-Thurgan and Wortmann, while in dairying the works of Duclaux and v. Freudenreich need only to be mentioned. While the importance of micro-organisms in the ripening of cheese is still a disputed question, their influence can scarcely be disregarded.

[1] Read before the Association of Economic Biologists, Edinburgh Meeting, July 29th, 1908.

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Even if it be admitted that the characteristic ripening of certain, or perhaps all kinds of cheese, is largely due to the activity of the natural enzymes of the milk, it is hardly possible not to ascribe to the bacteria at least some substantial part in the results. Even if bacterial influence should be shown to be of a negative nature only, we have still to reckon with the fact that the presence of lactic acid bacteria acts as a preventive against the development of the members of the putrefactive groups. The use of pure cultures in butter-making and recently in the manufacture of margarine, has given the most excellent practical results, of which the high reputation enjoyed by Danish butter affords a good example. This reputation very largely rests upon the uniformity of the quality, the natural result of the pure culture system.

Turning next to the greatest of all human industries—agriculture—we find that the questions which have so far chiefly engaged the attention of the technical mycologist are those connected with the bacteriology of the soil, and more especially the assimilation of free nitrogen, the nitrification and the denitrification. The first mentioned of these three questions is as yet very much in an experimental state, at least as far as practical results are concerned. The fixation of free nitrogen is of the highest importance in nature's economy, and the questions connected therewith are of such vital interest to the human race, that the discovery of the nodule bacteria of the *Leguminosae*, of the *Chlostridium pasteurianum* and the Azotobacter species, ranks among the most important scientific achievements of modern times. The apparent contradiction in the results of the practical experiments with pure cultures of nitrogen-fixing bacteria, finds its natural explanation in the pre-existence in the experimental fields of either sufficient nitrogenous food material for the plants or of an abundance of the necessary bacteria, or both. However, the question is only in its infancy, and presents a very promising field for further research, in the course of which it may possibly be found that the faculty of fixing free nitrogen is shared by numerous other organisms.

These few instances may suffice as examples of the direct utilisation of micro-organisms, or as it might be called the positive application of Technical Mycology. This science has, however, another side which may be called the negative one, consisting in the guarding against the inroads of micro-organisms which, by their development and activity, might exercise a more or less harmful influence upon the process of manufacture and the results of the industry. As examples may be mentioned sugar, starch, gelatine, and preserve works. Hereunder come the various processes of complete or partial sterilisation by heat, filtration, or chemical antiseptics, and, in a wider applica-

tion of the term, all the precautions against the spreading of infectious diseases of animals and plants. Pathological Mycology is, however, usually regarded as being outside the sphere of the Technical Mycologist, to which it can only be said to belong if fungi and bacteria are utilised directly to combat the disease-bringing organisms.

Of this we have a beautiful illustration in Metschnikoff's regulation of the bacterial flora of the alimentary canal by means of a systematic introduction of pure cultures of lactic acid bacteria.

This brings us on to the point on which I desire to say a few words, namely, the importance of the influence which micro-organisms exercise upon each other's development. The line which Technical Mycology has followed hitherto, and naturally must follow, has been the isolation of the most suitable species, variety, or type, for the particular industry in question, and an attempt to prevent complications by a rigorous exclusion of all others. The excellent results obtained in this manner have already been briefly alluded to and require no further comment, they speak for themselves. But it is nevertheless quite clear that if further substantial progress is to be made, not only in connection with the industries already within the domain of Technical Mycology, but if that science is to be extended to embrace other industries, the field of operations will require to be considerably widened. Just as we in the Algebra commence by considering the equations of the first degree, and gradually go on to the discussion of those of the second, third, and higher degrees, so we have in Technical Mycology commenced by mastering the problems of the first degree, those involving one species or variety of organism only, and are now face to face with the problems of the second and higher degrees, involving the simultaneous development of one or more either closely allied varieties or widely different species. These problems naturally fall into two classes. On the one hand we have such cases where any one species or variety does not carry out the entire amount of work required, and on the other hand we have such cases where the difficulties in sterilisation for one reason or another render the working of a pure culture, with the exclusion of admixtures, an impossibility. In addition may be mentioned those cases where the activity of micro-organisms is not in itself desired, but, as it cannot be avoided, an attempt is made to regulate it. As an example of the first class I should mention the brewing of British beers. Numerous, unfortunately unsuccessful, attempts have been made to carry through the fermentations of these by means of a single pure culture on the lines which have been such a great success in every other class of brewing. In a paper read before the Institute of Brewing, and afterwards in the Keith Lectures to the Royal Scottish Society of

Arts, I have expressed my views as to the lines on which this highly important and interesting question may be solved. These views are based upon numerous observations in practice as well as in the laboratory, indicating that when a mixture of micro-organisms is continually cultivated under the same set of conditions—food, temperature, time, etc., an equilibrium is soon established, after which the proportions in which the individual members are represented in the mixture are not changed to any appreciable extent. (It goes, of course, without saying, that this proportion may be, and in the majority of cases is, = o; this is, the variety in question is eliminated). The exact scientific proof of the correctness of this statement is, unfortunately, still wanting, owing to the almost prohibitive difficulties in identifying such large numbers of cultures, so closely resembling each other, as would be necessary in this case. However, I have the best hope that I shall, before long, be able to complete this part of the work. As examples of the second class, we have distilling and yeast making and cheese making. It is well enough known how the temperature, and, in the case of distilling and yeast making, the reaction of the wash is regulated in order to control the development of micro-organisms. In this last-mentioned case we have in addition an example of the utilisation of bacteria, which are not in themselves desired, but are harmless, for the purpose of preventing the development of others. I allude to the addition of lactic acid bacteria to prevent Butyric fermentation. Of further examples of this class we have Metschnikoff's use of lactic acid bacteria, to which I have already referred. The question I propose to treat separately, and I shall, therefore, just now proceed to state the conclusions to which I have come.

"As it is only in exceptional cases that it is possible to work in practice with pure cultures absolutely free from the admixture of other organisms, and as it is necessary for the successful carrying on of several industries that more than one species or variety be employed, it is essential that the conditions affecting the relative development in mixtures of micro-organisms receive the most close attention of the Technical Mycologists."

This necessity opens up an almost unlimited field closely connected with biochemistry. The problems would seem to resolve themselves largely into a study of the enzymes and anti-enzymes, and the toxins and anti-toxins, the conditions regulating the production and activity of these, and the manner in which the development of the individual members present in mixtures are thereby affected.

In many cases the problem is probably simple enough, as for instance the suppression of *Saccharomyces apiculatus* in the wine

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fermentation apparently by the large amount of alcohol produced by the wine yeast, or the prevention of putrefaction by the acid-forming species. But in numerous other cases the influences are undoubtedly of a far more subtle nature as, for instance, in the development of a British brewing yeast, or in the development of various bacteria in milk. It is true that if in this latter case the development of the lactic acid species is allowed to proceed sufficiently far to form an appreciable quantity of free acid, the retarding effect of this upon the development of a great many other organisms will be so strong that it will overshadow most other factors. I am, however, convinced from what I have seen during my work with bacteria in milk that there are other factors at work, and I propose to lay before you the results of my experiments so far as they go and incomplete as they are.

It is now a couple of years since Dr. A. P. Laurie called my attention to Metschnikoff's ideas, and suggested that I might be able to do something to have these introduced into this country. After having made myself acquainted with the literature on the subject, I proceeded to isolate a number of lactic acid bacteria from various sources. In estimating the relative value of these I used their power of retarding the development of a culture of *Bacillus coli communis*. I chose this method partly because it appeared to me that members of this group were the principal mischief makers in the alimentary canal, partly because it seemed evident that the suppression of members of the putrefaction groups would present no difficulties, and partly because the *Bacillus coli* varieties being themselves lactic ferments, would probably be in a position to offer more resistance towards the lactic acid bacteria than most others, so that a type of the latter which was able to combat successfully *Bacillus coli* might be confidently expected to come out victorious from the competition with most other species.

The culture which I finally adopted was a *Streptococcus* form obtained from a sample of butter from Normandy, and of this I supplied during nearly a year a large number of samples daily to several of my friends in the medical world. I do not propose to say anything about the medical aspect of the question, on which I am, of course, totally incompetent to speak, but I shall only refer to a publication by Dr. A. Veitch in the "British Medical Journal" (10th August, 1907), and I hope that others will soon be in a position to publish their results, which, I understand, are in many cases of a highly satisfactory nature.

In the following experiments the culture of *Streptococcus acidi lactici* already referred to was used. The culture of *Bacillus coli communis* was obtained by isolation from human faeces and developed in Glucose-Peptone Solution ($\frac{1}{2}$% Peptom Witte and 2% Glucose).

## EXPERIMENT (A).

Both cultures were developed for 24 hours at $37\frac{1}{2}^{\circ}$ C. in Glucose-Peptone. One drop of each of these cultures was added to a Freudenreich flask containing about 7 cc. Glucose-Peptone Solution, which was then placed in the incubator at $37\frac{1}{2}^{\circ}$ C. From this neutral Glucose-Peptone-Gelatine, plates were made after 24 hours and 48 hours incubation. One drop of the culture was diluted with 10 cc. sterile water. Of this dilution one drop was again transferred to 10 cc. sterile water, and from this one drop transferred to the Gelatine. After development the number of colonies of each type was counted, the difference in their appearance being of course sufficiently great to make this a comparatively easy, although rather tedious, piece of work. For further confirmation twelve colonies of each description were taken out of each plate in this and also in the following experiments, developed in Glucose-Peptone Solution and examined under the microscope.

### RESULTS OF COUNTING.

Plate made after 24 hours. Total number of colonies = 75.

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 6.6% |
| S.A.L. ... ... ... ... ... ... | 93.4% |

Plate made after 48 hours. Total number of colonies = 631.

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 12.8% |
| S.A.L. ... ... ... ... ... ... | 87.2% |

## EXPERIMENT (B).

Same as (A) with the exception that a large excess of calcium carbonate powder was here added to the Glucose-Peptone Solutions.

### RESULTS OF COUNTING.

Plate made after 24 hours. Total number of colonies = 2,800. (Only one-fourth of the plate counted).

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 8.8% |
| S.A.L. ... ... ... ... ... ... | 91.2% |

Plate made after 48 hours. Total number of colonies = 317.

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 11.3% |
| S.A.L. ... ... ... ... ... ... | 88.7% |

These results would seem to show:—

(1). That the S.A.L. possesses in a very high degree the power of checking the development of B.C.C.

(2). That during the initial, and probably decisive, stages in the competition, this power does not depend upon the formation of lactic acid, as in both experiments a slight increase in the percentage of B.C.C. colonies is observed in the 48 hours plates as compared with those made after 24 hours, and, further, the results are practically identical in both experiments, the presence of calcium carbonate having apparently no effect.

## EXPERIMENT (C).

The object of this experiment was to show to what extent the presence of B.C.C. in overwhelming numbers from the beginning of the experiment would affect the results. The Glucose-Peptone Solution was therefore infected with a drop of the B.C.C. culture alone, incubated for 24 hours at $37\frac{1}{2}^\circ$ C., and then a drop of the S.A.L. added. The culture was then again placed at $37\frac{1}{2}^\circ$ C., and plates made after 24 and 48 hours as before.

### Results of Counting.

Plate made after 24 hours. Total number of colonies = 473.

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 11.8% |
| S.A.L. ... ... ... ... ... ... | 88.2% |

Plate made after 48 hours. Total number of colonies = 1,374.

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 8.1% |
| S.A.L. ... ... ... ... ... ... | 91.9% |

From these results it will be seen that the large number of B.C.C. has not seriously affected the development of S.A.L., and it would seem justified to expect that if the conditions were reversed, if the B.C.C. had to compete against a very large majority of S.A.L., it would scarcely develop at all.

The results of experiments (A) and (B), however, seemed to indicate that the position might possibly be a more complicated one, and in order to try this the following experiment was carried out, in which the only difference from (C) was that in this case S.A.L. had the 24 start.

### Results of Counting.

Plate made after 24 hours. Total number of colonies = 90.

| | |
|---|---|
| B.C.C. ... ... ... ... ... ... | 5.5% |
| S.A.L. ... ... ... ... ... ... | 94.5% |

Plate made after 48 hours was lost by accident.

Taking the results of all four experiments together, the evidence seems to be in favour of the opinion that the retarding effect upon the

development of B.C.C. exercised by S.A.L. is not merely due to the formation of lactic acid, and would further seem to indicate that if mixtures of the two bacteria here mentioned are cultivated under conditions which preclude the formation of large quantities of free lactic acid, an equilibrium between the two species, approximately in the proportion of 1 to 9, would be established.

It seems quite possible that the power possessed by the lactic acid bacteria of retarding the development of other bacteria, or at least most of the bacteria in milk, could be utilised for the purpose of increasing the purity of our milk supply. The bacterial contents of milk, as it is now retailed, more especially in large cities, are such that improvements are very urgently needed, as is amply proved by practically every report from Medical Officers of Health. Excellent information on this subject is also contained in the report just issued by the Committee appointed by the East and West Ridings of Yorkshire and adjoining counties, in which it is clearly shown how much can be done by observing the most scrupulous cleanliness in the dairy farms as well as in the retailers' and consumers' places; but the question still remains whether a milk supply obtained under the most ideal conditions that would be practicable, could be safely regarded to be without risk of causing or spreading disease. The remedy which has so often been advocated and tried—but so far with very little success—is that of sterilising, or partly sterilising the milk. The great objection to this process is that milk so treated will still contain a fairly large number, especially of spore-forming bacteria, which, when the milk is kept, will develop side by side with those bacteria which reach the milk after the sterilisation process. As the lactic acid bacteria have all been killed, such milk will not become sour and thereby indicate that it has been kept too long, in fact a sample of so-called sterilised milk may perfectly well be swarming with bacteria without exhibiting any sign of their presence, at least not to the eye of the ordinary consumer, who has been accustomed to look upon any sample of milk that is not sour as being good. What calamities can be, and undoubtedly are being caused by the use of such milk which is actually in a state of decomposition, can better be imagined than described. If the milk, however, had received immediately after the sterilisation process just a trace of a pure culture of lactic acid bacteria specially selected for the purpose, this great objection and danger would have been entirely done away with. The presence of these bacteria would not only retard the development of others, but would ultimately, by their own development, indicate when the milk had been kept too long; and quite apart from the question of sterilising the milk, it would seem quite possible to

improve the purity of the natural milk by the addition of such cultures immediately after the milking process. The cost of this, if it were carried out regularly, would be quite insignificant, and the presence of the lactic acid bacteria in the milk from the very outset would probably afford the best safeguard against the development of the various more or less undesirable species.

Before concluding, I think I should mention that the experiments which I have described were not carried out with a view to publication, at least not in their present form, but when I was requested a few weeks ago to make some remarks at this meeting, it occurred to me that the results, and the ideas which they seem to suggest, might be of interest to some of those present.

## NOTES.

### Preliminary Note on the Action of Yohimbine on the Generative System.

BY

**W. CRAMER, Ph.D., D.Sc., and F. H. A. MARSHALL, M.A., D.Sc.**

*(From the Physiology Department, University of Edinburgh.)*

Seeing that the drug Yohimbine is commonly stated by veterinarians to act as an aphrodisiac, and that it has been claimed by some to be capable of inducing a condition of "heat" in animals and to be an effective remedy for certain kinds of sterility, it seemed desirable to undertake a systematic investigation upon the precise nature of the action of this drug on the female generative organs. We first administered it to two small anoestrous bitches, the date of the preceding "heat" period having been noted for one of them, but not for the other. Each animal received about .005 grams of Yohimbine twice daily, for nearly a fortnight, the drug being administered in the form of tablets, which were eaten. A marked congestion of the vulva resulted, especially in the case of the dog whose previous "heat" period had been noted to occur a few weeks previously.

After ceasing to administer the drug the effects passed off, and the vulva once more became pale. This result agrees with that obtained by Daels ("Surgery, Gynaecology and Obstetrics," February, 1908).

We then proceeded to investigate the effects of Yohimbine on the generative organs of rabbits. These animals received twice daily doses of .005 grams by the mouth in the form of tablets. The external generative organs became very deeply congested after a few days. Moreover, it was found on killing the rabbits that the uterus and entire generative tract were also congested, sections showing that the vessels were much engorged with blood. It also appeared that the uterine mucosa had undergone growth in consequence of the treatment, but whether these changes are to be regarded as truly prooestrous must still remain an open question. The ovaries were much overgrown by luteal tissue, and degenerate follicles, which are generally so common in rabbits' ovaries, were relatively scarce. It seems extremely probable, therefore, that Yohimbine, by preserving a constant and rich supply of blood, and consequently of nutriment, to the ovaries, may arrest the normal process of follicular degeneration, and so be the means of bringing a larger number of follicles to maturity than would otherwise be the case, thereby tending to increase the fertility. There was no evidence, however, that Yohimbine by itself is capable of

inducing ovulation in the rabbit, this animal differing from most in its failure to ovulate, except as a result of a nervous reflex set up by sexual intercourse.

Lastly, evidence was adduced that Yohimbine may promote mammary development and the secretion of milk, since in five rabbits to which the drug was administered milk was found in considerable abundance in the glandular tissue, in spite of the fact that the animals had not recently been suckling; while in another virgin rabbit there was distinct evidence that Yohimbine had promoted a hypertrophy of mammary tissue to an extent at least as great as that observed by Miss Lane-Claypon and Starling after the injection of foetal extract. It will, however, be necessary to confirm this observation before we can speak more definitely in regard to the action of Yohimbine on the mammary glands.

## A Note on Abortion as a Result of a Diet rich in Carbohydrates.

BY

**W. CRAMER, Ph.D., D.Sc., and F. H. A. MARSHALL, M.A., D.Sc.**

*(From the Physiology Department, University of Edinburgh.)*

In a recent paper (Proc. Roy. Soc., B. 1908), Lochhead and Cramer showed that in the pregnant rabbit there is a distinct relation between the amount of glycogen in the placenta and the growth of the foetus. A diminution in the glycogen of the placenta, whether occurring spontaneously or produced experimentally, was accompanied by a diminution in the weight of the foetus. The amount of glycogen present at any one day of pregnancy was found to be remarkably constant, and could not be increased by feeding the pregnant animals on a diet rich in carbohydrates (carrots). It was noted, however, that out of six pregnant animals which were kept on such a diet, three aborted.

In the present investigation the effect of such a diet was tested again on twelve female rabbits, which were kept intermittently with the same buck. Six of them were then fed on cabbage and carrots, while six others were fed on cabbage and oats, the latter serving as controls. Of the six control animals all had normally developed young ones. Of the six rabbits fed on carrots, three aborted at different stages of pregnancy.

This result agrees with the experience of many stock owners, that cows fed on molasses prove to be uncertain breeders (See Wallace: "Farm Live Stock," 1907), and that Lincoln sheep fed solely on turnips are especially liable to abortion (Heape, Journ. Roy. Agricultural Soc., 1899), but the last-mentioned fact has been ascribed by Heape to the fouling of the roots by mud and excrement, a condition of things which results from overcrowding.

## REVIEWS.

**Bailey, L. H.**—The Horticulturists Rule Book. Pp. ix + 312. Toronto: The Macmillan Company of Canada, Ltd., 1908.

This is a special edition of Professor Bailey's well-known handbook printed for the Government of British Columbia, for distribution amongst members of the Farmers' Institute.

It is cram full of valuable and useful information bearing upon insecticides, injurious insects, plant diseases, injuries from mice, rabbits, birds, etc., weeds and moss, seed-tables, planting-tables, and information upon the methods of keeping and storing fruits and vegetables, in addition to a large series of tables, elements, symbols, analyses, etc., etc.

We commend this handy reference book to all horticulturists and gardeners.

W. E. C.

**Henslow, G.**—The Heredity of Acquired Characters in Plants. Pp. xii + 107. London: John Murray, 1908. Price 6s. net.

The Rev. George Henslow, in this volume, maintains the thesis that evolution in plants depends on the inheritance of characters which have their origin in direct adaptation of the organisms to new environments; adaptations which become fixed or hereditary if the plants live long enough, generation after generation, in their new surroundings. Opposed to this is the view that variations are congenital, appearing in the seedlings, the unfit being eliminated, whilst the fittest survive, a view in harmony with Weismann's theory of the germ plasm.

Numerous examples in support of the author's view are marshalled from such groups of plants as the xerophytes (drought-loving plants), climbers, aquatics, etc. It is argued that in the xerophytes the succulent habit is a direct adaptive response on the part of the plant to the new environment, and that through successive generations this succulency becomes hereditary. In this, as in his earlier book, "The Origin of Plant Structures by Self-Adaptation to the Environment," the author appears to consider one factor to the exclusion of others. The environment cannot alone be the direct cause of the succulent habit, because examination of various plants, growing under the same conditions, shows that whilst some plants are succulent, others have underground bulbs and rhizomes, hairy leaves, a spinous habit, an extraordinary development of the root system, and so on. In other words, the same environment produces very dissimilar results, according to the personal equation of the plant and its response to tendencies already hereditary and not directly induced *de novo* by the action of the environment alone.

It is impossible, in a brief notice such as this, to do full justice to a book which, with the author's previous works, will be appreciated by botanists for the wealth of interesting examples cited, even although in all cases we find ourselves scarcely able to accept fully his conclusions.

The book is well produced and illustrated.

W. F. G.

**Hunting, William.**—Glanders, A Clinical Treatise. Pp. 105, with 17 plts. London: H. and W. Brown, 1908. Price 10s. 6d. net.

Veterinary science, and pathologists in particular, are much indebted to the author for this beautifully illustrated treatise. No one in this country is more capable or better qualified than Mr. Hunting to write on the subject of glanders, for he has taken a keen interest in, and has been practically associated with, the disease for now nearly a quarter of a century.

Modestly, but with a dogged pertinacity, he has spared no effort to keep the importance of the subject before succeeding Governments, and to impress upon them the seriousness of the disease to the health and wealth of the nation.

The disease is lucidly described, its history, etiology, symptoms, post-mortem appearances, diagnosis, the mallein test, cure and recovery, prevention and legislation, with an important appendix on glanders in man.

The work cannot fail to help those whose experience is small, in providing them with the fullest information, and will assist them to detect and suppress the disease should they meet with it.

There is little doubt but that so valuable a contribution to the literature of veterinary pathology will find a place in the library of everyone who has to deal with diseases of the horse. It is ably written, and leaves nothing to be desired in the manner in which it has been presented to the public.

W. E. C.

**Neumann, L. G.**—Parasites et Maladies Parasitaires des Oiseaux Domestiques. Pp. viii + 230, 89 text figs. Paris: Asselin et Houzeau, 1909.

In a remarkably small compass Professor Neumann has given an admirable description of the parasitic diseases of fowls and other domestic birds.

The descriptions of the various parasites are concise, lucid and well-illustrated throughout, whilst the inclusion of the common and scientific names and authorities make this a most useful handbook.

No pains seem to have been spared to make it as thorough and complete as possible.

W. E. C.

**Pickering, S. U., and Theobald, F. V.**—Fruit Trees and their Enemies, with a Spraying Calendar. Pp. i + 113. London: Simpkin, Marshall, Kent and Co., Ltd., 1908. Price 1s. 6d. net.

It is difficult to imagine what induced the authors to compile the little work before us. All that it contains has been said before and said much better, whilst its brevity, in many cases, detracts from its value.

The much debated "Woburn Washes" are strongly recommended, but up-to-date fruit growers will, we feel sure, hesitate before they treat their trees with caustic soda emulsions and paraffin mixtures. As is now well-known both caustic soda and paraffin do considerable harm to plant life, and are only partially successful in the destruction of insect pests; apart from this fact, their price is beyond the reach of the man who grows for profit.

The injunction to collect and burn all fallen leaves, as they may be infested with eggs, etc., of injurious insects, fungus spores, etc., if carried out, would mean two to three months' hard work for a small army of men, which practical fruit-growers will smile at. Such an injunction as this is most unfortunate, coming as it does from the advisers of the Duke of Bedford's Experimental Fruit Farm and the Wye Agricultural College.

W. E. C.

**Fuhrmann, O.**—Die Cestoden der Vögel. Zool. Jahrb. Suppl. 10, plt. i, Pp. 232. Jena: Gustav Fischer, 1908.

Dr. Fuhrmann has written a most interesting work, both from the standpoint of the parasitologist as well as ornithologist. The fact that the different parasites bear a distinct relationship to the different groups of birds is one worthy of further study.

It is, however, as a treatise on parasitology that it commends itself to our notice. As is fairly well-known the Cestodes are especially numerous amongst birds, and present many widely differing types. No less than sixty-four generic types are here treated of, together with the particular group of birds in which they are found. In many cases these generic types include a large number of species, all of which are set forth with full details.

The work is of considerable value to the parasitologist, and cannot fail to interest economic biologists generally.

W. E. C.

## CURRENT LITERATURE.

### I.—General Subject.

### II.—Anatomy, Physiology, and Development.

**Bordas, L.**—Recherches sur les glandes défensive ou glandes odorantes des Blattes. Ann. d. Sci. Nat. (Zool.), 1908, pp. 1-25, pl. i.

**Bruntz, L.**—Les reins labiaux et les glandes cephaliques des Thysanoures. Arch. Zool. exp. et gen., 1908, pp. 195-238, pls. ii, iii.

**Carter, R. M.** A Preliminary Note on Spirochaetosis in Southern Arabia and the Morphology of the Parasite. Indian Med. Gaz., 1908, pp. 370-374, plts. i-v.

**Felt, E. P.**—Circumfili of the *Cecidomyiidae*. New York State Mus., Bull. 124, 1908, pp. 305-307.

The peculiar antennal structures first discovered by Targioni-Tozetti in 1888, are most highly developed in the male Diplosids, where they consist of nearly homogenous whorls of long, looped filaments extending around the enlargements of the segments. These structures occur practically in all the *Cecidomyiinae*, but are not present in the *Lestreminae*. In the genus *Lasioptera* they are extremely simple in form, and in *Cincticornia* they present somewhat remarkable modifications, whilst the most unique type is found in the genus *Winnertzia*.

The author puts forward the view "that these organs may be hypodermal structures, which, through a process of development, have migrated from the interior of the antennal segment, becoming external, and thus greatly increased their value as auditory organs." An alternative explanation is that they are simply specially modified setae, and this is the view we prefer to take until further evidence is forthcoming.

**Hewitt, C. Gordon.**—The Structure, Development and Bionomics of the House-fly, *Musca domestica*, Linn. Pt. II. The Breeding Habits, Development and Anatomy of the Larva. Quart. Journ. Micro. Sci., 1908, pp. 495-515, plts. 30-33.

**Minchin, E. A.** Investigations on the Development of Trypanosomes in Tsetse-Flies and other Diptera. Quart. Journ. Micro. Sci., 1908, pp. 159-260, plts. 8-13, and 2 text figs.

**Nuttall, G. H. F., Cooper, W. F., and Robinson, L. E.** On the Structure of "Haller's Organ" in the *Ixodoidea*. Parasitology, 1908, vol. i, pp. 238-242, plt. xviiii, 1 text fig.

From an examination of the structure of this organ, which is borne on the dorsal surface of the tarsus of the first pair of legs, the authors come to the conclusion that it is not auditory in function, but, as advanced by Lahille in 1905 from experiments, olfactory.

The minute structure is now described for the first time, and this and Lahille's experiments are all strongly in favour of the assumption that the organ is olfactory in function.

**Patton, W. S.**—Preliminary Report on the Development of the Leishman-Donovan Body in the Bed Bug. Sci. Mem. Off. Med. San. Dept. Govern. India, 1907 (n.s.), No. 27, pp. 1-19, 1 plt.

**Thompson, O. S.** Appendages of the Second Abdominal Segment of Male Dragon Flies (Order Odonata). New York State Mus., Bull. 124, 1908, pp. 249-263, figs. 17-28.

## III.—General and Systematic Biology, and Geographical Distribution.

**Chadwick, G. H.**—A Catalogue of the "Phytoptid" Galls of North America. New York State Mus., Bull. 124, 1908, pp. 118-155, plt. 3.

The author gives a list of host plants on which leaf-galls occur. We fail to see the value of such. A list of the *Eriophyidae* of North America would have been extremely useful.

**Cockerell, T. D. A.** A Remarkable Cecidomyiid Fly. Canad. Entom., 1908, pp. 421, 422.

Under the name of *Hormomyia coloradensis*, n.sp., the author describes a somewhat peculiar Cecidomyiid, in which the third vein of the wing is continued straight to the base of the fifth, and a little cross-vein to the first, which is supposed to be the real beginning of the third, is totally absent.

**Felt, E. P.**—New Species of *Cecidomyiidae. II.* New York State Mus., Bull. 124, 1908, pp. 286-304.

Fifty-one new species are described.

**Felt, E. P.**—Studies in Cecidomyiidae. II. New York State Mus., Bull. 124, 1908, pp. 307-422, plts. 33-44, figs. 29-49.

A valuable paper containing much new information respecting the American *Cecidomyiidae* and the Family as a whole.

The following genera are described as new:—*Microcerata, Neolasioptera, Camptoneuromyia, Diarthronomyia, Walshomyia, Sackenomyia, Cincticornia, Dentifibula, Lobodiplosis, Karshomyia, Youngomyia, Pro-*

*diplosis*, *Odontodiplosis*, *Adiplosis*, *Hyperdiplosis*, *Giardomyia*, *Metadiplosis*, *Epidiplosis*, *Paradiplosis*, *Obolodiplosis*, and *Johnsonomyia*. Many of these are founded upon little more than specific characters, and we regret that the author has not, at all events for the present, included them in existing genera.

**Girault, A. A.**—The Oviposition of *Chilocorus bivulnerus*, Mulsant. Journ. Econ. Entom., 1908, vol. i, pp. 300-302.

**Houghton, C. O.**—Notes on the Lesser Clover-leaf beetle (*Phytonomous nigrirostris*, Fab.). Journ. Econ. Entom., 1908, vol. i, pp. 297-300.

**Howard, C. W.**—A List of the Ticks of South Africa, with Descriptions and Keys to all the forms known. Ann. Transv. Mus., 1908, vol. i, pp. 73-169, plts. i-xvi.

Although describing no new species, this is a most useful and valuable paper. In addition to giving full diagnostic characters, and numerous figures, the author includes a bibliography of South African ticks and a list of the animals which act as hosts for the same.

**Johannsen, O. A.**—New North American *Chironomidae*. New York State Mus., Bull. 124, 1908, pp. 264-285.

**Kirby, W. F.**—A Gall-producing Dragon-Fly. Nature, 1908, p. 68.

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**Needham, J. G.**—New Data concerning May Flies and Dragon Flies of New York. New York State Mus., Bull. 124, 1908, pp. 188-248, plts. 10-32, and figs. 5-16.

**Neumann, L. G.**—A new species of Tick from the Transvaal. Ann. Transv. Mus., 1908, vol. i, pp. 170-172.

The new species is named *Rhipicentor vicinus*.

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**Pierce, W. D.**—Factors controlling Parasitism with special reference to the Cotton Boll Weevil. Journ. Econ. Entom., 1908, vol. i, pp. 315-323.

**Scott, Hugh.**—On certain *Nycteribiidae*, with descriptions of two new species from Formosa. Trans. Entom. Soc. Lond., 1908, pp. 359-370, plt. xviii.

**Silvestri, F.**—Liste des *Japygidae*. Ann. d. Sci. Nat. (Zool.), 1908, pp. 151-157, 17 figs.

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**Wesché, W.**—The systematic affinities of the *Phoridae* and of several Brachycerous families in Diptera. Trans. Entom. Soc. Lond., 1908, pp. 283-296, plt. vii.

IV.—Agriculture and Horticulture.

**Berger, E. W.**—The Citrus Whitefly of Florida consists of two species. Journ. Econ. Entom., 1908, vol. i, pp. 324, 325.

**Chittenden, F. H.**—The Strawberry Weevil (*Anthonomus signatus*, Say.). U.S. Dept. Agric., Bur. of Entom., Circ. No. 21, rev. ed., 1908, pp 1-10, 5 figs.

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A report full of valuable information. Apart from the account of the excellent experimental work, there are numerous life-histories detailed and beautifully illustrated.

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### VI.—Fisheries.

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